

7 - TAZ - 2/77  
November 2, 1972

Dr. Alfred E. Mirsky  
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New York, New York 10021

Dear Alfred,

Thank you for your letter of October 31. Olby is writing a much longer historical account which I hope will do a little better than his letter in Nature. In it, among other things, he quotes extensively from Dubos' biographical memoir of Avery which appeared in the Royal Society's series in 1956. This makes clear, what I had not realized, how independent Dawson's work was and in fact that Avery took some persuading to take transformation seriously.

On page 2 you refer to Rogers (1932) -- a name I cannot identify in this context.

My reference to the lapse of work after Griffith was really focussed on the monopoly that seemed to be enjoyed by the Rockefeller group, within which I was perhaps not adequately discriminating. The delay in bringing other markers into play certainly sustained a confused image of what was going on in transformation for many years. I remember your own remarks in 1950 and they also resonated with what Muller had written in his program lecture. In 1951 at the Cold Spring Harbor Symposium we thought we were adding to this dimension by correlating transduction in Salmonella with E. coli recombination and with the pneumococcus transformation. Of course, in hindsight it is very difficult to reconstruct the very vague ideas that were eventually displaced by these rather crisp notions of genetic transfer.

I would look forward to a more complete account that you mention at the close of your letter.

Dubos' biography refers to a talk that Avery gave at the institute sometime in 1943. I wonder if it would be possible to unearth the exact date of that presentation.

Sincerely yours,

Joshua Lederberg  
Professor of Genetics

JL/rr